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Whether, When, and Why Skinner Published on Biological Participation in Behavior

Edward K. Morris, Junelyn F. Lazo, and Nathaniel G. Smith
University of Kansas

This paper brings some data to bear on the criticisms, claims, and arguments that Skinner (a) denied or dismissed biological participation in behavior, (b) addressed it only late in his career or more often later than earlier, or (c) addressed it only because of the overwhelming evidence for it or the criticisms that he had overlooked it. For this, we coded Skinner's every primary-source publication for three content categories (i.e., genetics, physiology, and evolution) and for the extent to which he addressed them (i.e., in publication titles, substantively, or in passing). Our findings are that Skinner addressed biological participation in over a third of his publications throughout his career, albeit more often later than earlier. The latter, however, is accounted for by an increase in his base rate of publication and in general conditions and specific events in his career, psychology, and science. The discussion addresses our research methods; the reasons for and refutations of the criticisms, claims, and arguments; and their sources.

Key words: B. F. Skinner, biology, genetics, physiology, evolution, ethology, misrepresentation

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B. F. Skinner (1904–1990) and, by nontrivial association, contemporary behavior analysis are criticized today for denying or dismissing the participation of biology in behavior. As the linguist and public intellectual Pinker (2002) recently remarked, "Behaviorists believed that behavior could be understood independently of the rest of biology, without attention to the genetic make-up of the animal or the evolutionary history of the species" (p. 20). He referred to Skinner as "a staunch blank slater" (p. 169; see also "Steven Pinker," 2004). In turn, the historian of psychology Leahey (2001) has written, "If there is no human nature, if human beings are mere clay to be shaped by society, then as John Watson and B. F. Skinner said later, we can make human beings to order" (p. 50). Many other prominent ethologists, primatologists, and psychologists agree (e.g., de Waal, 1999, p. 97; 2001, p. 57; 2004, p. 53; Gould, 1982; Gould & Marler, 1987; Tiger & Fox, 1971, pp. 11–12; see also Mahoney, 1989; Yoerg, 2001, pp. 76–77, 79–80, 133–134). Garcia (1981), for example, criticized Skinner often for seeing "no need to concern ourselves with species differences, with brain differences, or with reinforcing differences" (p. 155; Garcia & Garcia y Robertson, 1985). Others have even stated that Skinner avoided "terms or concepts dealing with organismic variables" (Blanchard, Blanchard, & Flannelly, 1984, p. 681).

Among the critics who acknowledge that Skinner did address biological participation, some claim that he only did so late in his career (e.g., Delius, 1984; Wahlsten, 1984). For instance, in commenting on Skinner's paper, "The Phylogeny and Ontogeny of Behavior" (Skinner, 1966a), Barkow (1984, p. 681) remarked that Skinner "now seems to understand and accept" that his system must be consistent with evolutionary biology. Garcia (1993) later wrote that Skinner only came to describe the relation between natural selection and operant conditioning "to-

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Correspondence may be sent to the first author at the Department of Applied Behavioral Science, University of Kansas, 1000 Sunnyside Avenue, Lawrence, Kansas 66045 (e-mail: ekm@ku.edu).

ward the end of his career" (p. 1158; see Bailey & Bailey, 1980). Skinner (1983b, p. 367) himself cited a 1981 article in the *Economist* that stated he "no longer opposed" physiological research.

Other critics claim that, even if Skinner did address biological participation over the full course of his career, he did so more often and more substantively later than earlier (e.g., Plotkin, 1987). Barash (1984), for instance, commented on Skinner's 1966 article this way: "Skinner . . . shows much greater sensitivity to species differences than I had previously appreciated, and in fact, more than he had shown in earlier writings" (p. 680). As for why Skinner addressed biological participation more often later than earlier, some critics argue that the evidence so overwhelmingly contradicted his views that he had to acknowledge it or had to accede to the critics who pointed this out. Burghardt (1984), for example, claimed that Skinner's 1966 paper was "a belated attempt to defend a system of thought against the growing evidence from ethology that something was awry in his elegant psychological world view that could no longer be ignored" (p. 683; Baerends, 1984; cf. de Waal, 2001, p. 57).

The foregoing criticisms are widespread, the claims are profound, and the arguments need to be taken seriously. If they are true, then Skinner and his system of psychology were and are deeply remiss. Where he advanced a system for understanding the behavior of organisms as members of a species and as individuals, that is, as historical entities—which he did (e.g., Skinner, 1953, 1974)—then he would have to include all the variables of which behavior is a function, evolutionary and biological variables included. However, if Skinner's critics are wrong, then this is a great hindrance to professional relations and collaboration across research programs in the behavioral, social, and cognitive sciences, and is mischievous in its effects on mutual understanding and respect among colleagues. The purpose of our paper is to bring some data to bear on these matters, specifically, (a) whether Skinner denied or dismissed biological participation in behavior, even to the point of avoiding organismic terms and concepts; (b) when he addressed them, if he did; and (c) why he addressed them later than earlier, if he did.

METHOD

We reviewed the 289 primary-source works Skinner published over the course of his career (1930 to 1990) listed in Morris and Smith's (2003) bibliography. We noted their titles, perused their content, consulted their indexes, along with others (e.g., Epstein & Olson, 1984, 1985; Knapp, 1974, 1975), and coded each publication for whether Skinner addressed biological participation in behavior. We also coded his publications for (a) three content categories—genetics, physiology, and evolution—and (b) the extent to which he addressed them: in the titles of his works, in substantive discussions, or with just a mention in passing. In every case, we coded only Skinner's constructive comments on biological participation, not his critiques; for instance, we did not code his criticisms of the conceptual nervous system and physiological reductionism (e.g., Skinner, 1938, pp. 418–432; 1953, pp. 27–31; 1974, pp. 213–224; see Schaal, 2003).

Content and Extent

In coding the content of what Skinner wrote, we coded (a) *genetics* if he used the following terms: genes, heredity, inheritance, instincts, their cognates, or examples thereof; (b) *physiology* if he used anatomy, brain, the nervous system, hormones, physiology, or related terms, but not the term reflex; and (c) *evolution* if he used ethology, evolution, natural selection, speciation, or the like. When he addressed more than one category in a publication, we coded each category, but only once, no matter how many

times he addressed it. If he addressed more than one category in a passage, paragraph, or section, we coded only the dominant category. When *physiology* included both endogenous and exogenous variables, we coded only the former, thus excluding injury to the nervous system (e.g., Skinner, 1931, p. 442), asphyxiation and disease (e.g., Skinner, 1938, p. 416), pharmacological agents (e.g., Heron & Skinner, 1937; Skinner, 1959a; Skinner & Heron, 1937), and experimentally induced brain lesions (e.g., Ferster & Skinner, 1957, pp. 85–89, 322–325, 577–579).

As for the extent to which Skinner addressed biological participation, we coded whether the titles of his publications included the foregoing terms, whether he addressed biological participation in a substantive manner (i.e., a full paragraph), or whether he just mentioned it in passing. When we coded a publication for its title, we did not code it again for discussions or mentions in the same category. Likewise, when we coded a publication for a substantive discussion, we did not code it again for any further mentions in the same category.

WHETHER SKINNER PUBLISHED

Our main findings were these: Skinner addressed biological participation in 97 (or 34%) of his 289 primary-source publications, including those in the first and last years of his career, and in 46 years (or 75%) of his 61-year career. Across these publications, he addressed genetics, physiology, and evolution on 133 occasions—genetics 61 times, physiology 36 times, and evolution 36 times. He included biological terms or concepts in the titles of 17 works, substantively discussed biological participation in 22 others, and mentioned it in 94 more.

Figure 1 presents these findings in cumulative records graphs. In Panel A, from top to bottom, the cumulative curves are for Skinner's primary-source publications (total: 289), the occasions on which he addressed biological participation (occasions: 133), the number of publications in which he addressed them (biology: 97), and then the occasions on which he addressed genetics (61), physiology (36), and evolution (36). Panel B presents the findings for the extent to which he addressed biological participation, with curves for all occasions, followed by the mentions (94), substantive discussions (22), and titles (17). Panel C sharpens the resolution of Panel A by graphing the three content categories against all occasions. Here, we see that Skinner's publication rate was not seamless, a point we address later. First, though, we describe the extent to which he addressed genetics, physiology, and evolution, organizing these categories by titles, discussions, and mentions, and offering some representative quotations.

Genetics

Skinner addressed genetics in the titles of five works, discussed it in nine others, and mentioned it in another 47, in both the first and last years of his career (Skinner, 1930a, 1990c).

Titles. In the five publications coded for titles (Heron & Skinner, 1939, 1940; Skinner, 1930b, 1971a, 1988), Skinner noted the constructive role that genetics plays in behavior. For example, in "On the Inheritance of Maze Behavior," a critique of Vicari (1929), he wrote, "That there are gross characteristics of behavior which show genetic constancy is of course, common knowledge to any stockbreeder. And that such gross characteristics must have influenced the 'reaction time' taken in the present experiment is fully evident" (Skinner, 1930b, p. 344). In "Genes and Behavior," he concluded that "the whole story will eventually be told by the joint action of the sciences of genetics, behavior, and culture" (Skinner, 1988, p. 83).

Discussions. Skinner's nontitled substantive discussions of genetics are found throughout his major texts and

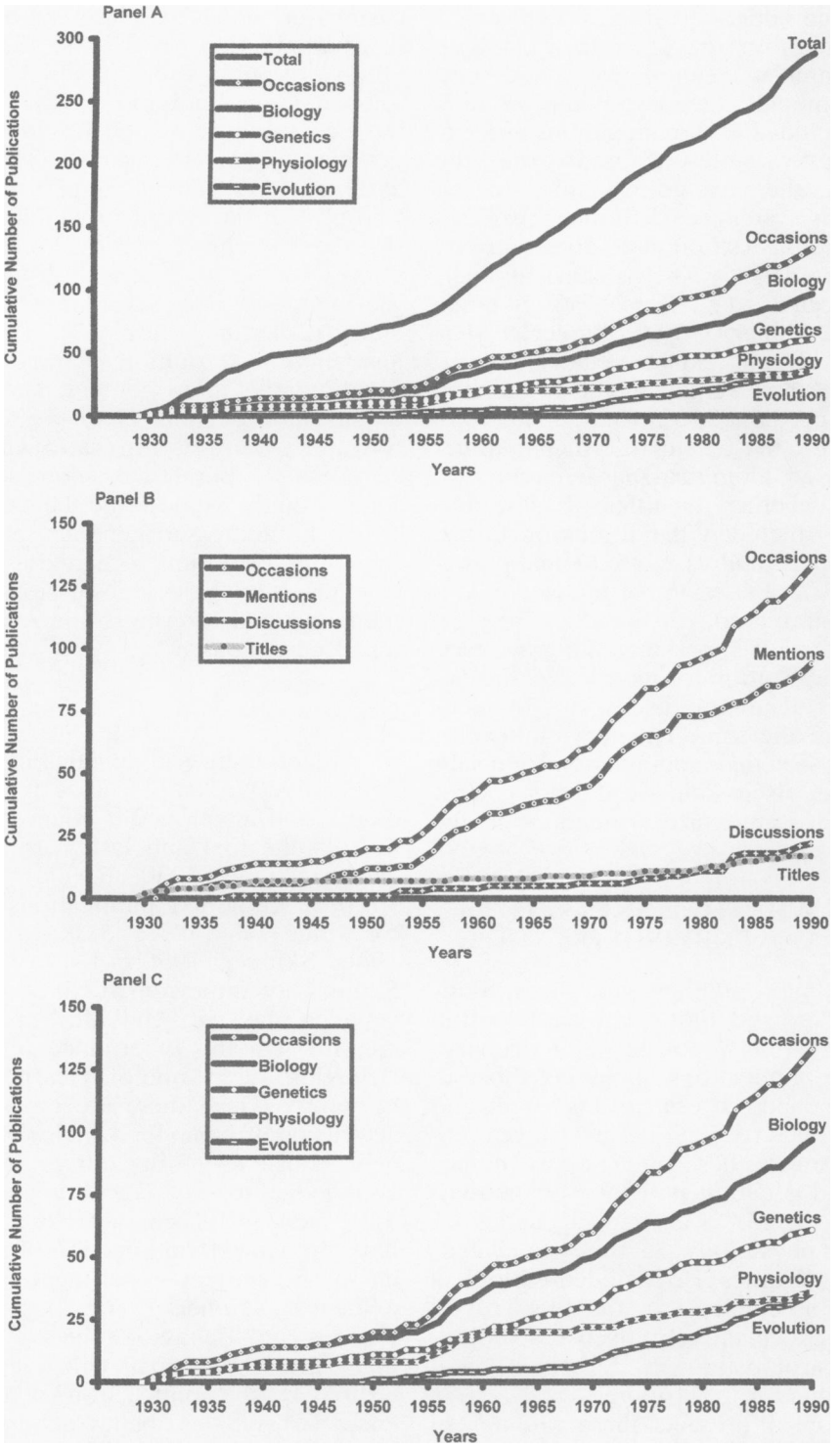

Figure 1. Cumulative records of Skinner's total publications and those that addressed biological participation in three content domains (Panel A), the occasions on which he addressed them broken into the extent to which he did so (Panel B), and the occasions on which he addressed them broken into the domains of his having done so (Panel C).

in his comments on them. For instance, of *The Behavior of Organisms* (Skinner, 1938), he (1984a) wrote, "Most of the experiments reported [there] were done with rats from the Bussey strain used by William Castle in studying mammalian genetics. I used them because I hoped to find behavioral differences which could then be treated genetically" (p. 701; see also Skinner, 1979, pp. 232, 238; 1983b, p. 102; 1984a, p. 703). Later, in *Science and Human Behavior* (Skinner, 1953), he pointed out that "Behavior requires a behaving organism which is the product of a genetic process. Gross differences in the behavior of different species show that the genetic constitution, whether observed in the body structure of the individual or inferred from a genetic history, is important" (p. 26; see also Skinner, 1983b, p. 13), to which he added, "Differences in hereditary endowment, which are too conspicuous to be overlooked when we compare different species, but presumably are also present to a lesser extent between members of a single species, account for other differences in repertoire" (p. 196; see also p. 25; Skinner, 1956b, p. 81). He even criticized J. B. Watson (1924) for "questioning the importance of many genetic factors" (Skinner, 1983b, p. 191; see Skinner, 1959c, on Watson). One of Skinner's (1969a) most extended discussions of genetics appeared in notes he published in *Contingencies of Reinforcement: A Theoretical Analysis*:

> A complete inventory of the genetic behavioral endowment of a given species would cover all aspects of its behavior in all possible environments, including (1) Skeletal and autonomic reflexes to all possible eliciting stimuli, including emotional responses under the most extreme provocation. (2) All instinctive responses evoked by identifiable releasers in all possible settings. . . . (3) All the behaviors which may be shaped and maintained by various contingencies of reinforcement, since a species is characterized in part by the positive and negative reinforcers to which it is sensitive and the kinds of topography which are within reach. . . . The speed, order, and direction in which a repertoire can be modified under operant conditioning are also presumably characteristic of a species. . . . (4) Behavior exhibited under unusual or conflicting sets of contingencies, particularly those involving punishment. (A disposition to neurotic or psychotic behavior and the forms taken by that behavior presumably vary across species.) (5) Behavior characteristic of all levels of deprivation. (pp. 201–202; see Skinner, 1983b, p. 13; 1984a, p. 506)

Mentions. When Skinner mentioned genetics only in passing, we still found relevant comments. In 1947, for instance, he observed,

> To have a science of psychology at all, we must adopt the fundamental postulate that human behavior is a lawful datum, that it is undisturbed by the capricious acts of any free agent—in other words, that it is completely determined. The genetic constitution of the individual and his personal history to date play a part in this determination. (Skinner, 1947, p. 23; see also Skinner, 1964a, p. 485)

For Skinner, the organism's genetic constitution played a significant role in both its general and specific behavioral repertoires. With respect to the former, for instance, Skinner (1948, p. 117) noted in *Walden Two* that even though the community's mean IQ was raised through its educational practices, the range of the scores remained the same. That is, the practices did not affect any genetically based individual differences (see also Skinner, 1961b, 1964b, on the genetic basis of individual differences in learning). About more specific repertoires, Ferster and Skinner (1957) observed that "pecking has a certain genetic unity; it is a characteristic bit of behavior which appears with a well-defined topography" (p. 7; Skinner, 1953, p. 93). As Skinner wrote later, "operant conditioners have never been concerned with teaching pigeons '*how* to peck.' The pecks come ready-made" (Skinner, 1980b, p. 53).

We summarize Skinner's comments on genetics with a final quotation: "All behavior is due to genes, some more or less directly, the rest through the role of genes in producing the structures which are modified during the lifetime of the individual" (Skinner, 1984a, p. 704).

Physiology

Where Skinner referred to physiology, he addressed it in five titles, discussed it substantively in five other publications, and mentioned it in another 27. These publications began in 1931 (Skinner & Crozier, 1931) and continued into the last year of his career (Skinner, 1990b).

Titles. Among the five works whose titles referred to physiology (e.g., Heron & Skinner, 1937; Skinner, 1958), three of them were among his earliest publications, for instance, an article on geotropisms (Barnes & Skinner, 1930), a review of Fearing's (1930) *Reflex Action, a Study in the History of Physiological Psychology* (Skinner & Crozier, 1931), and a paper on conditions that affect motor nerve thresholds (Lambert, Skinner, & Forbes, 1933).

Discussions. Skinner's first substantive discussion of physiology was his treatment of what he called "third variables" (Skinner, 1931), that is, variables that affect the functional relations between stimuli (the first variables) and responses (the second variables). The third variables he discussed most systematically were "conditioning, drive, and emotion," the last two of which he later said would be called "organismic" by those who criticized him for avoiding such terms and concepts (Skinner, 1984a, p. 702). He also included as third variables more straightforward biological factors such as the integrity of the blood supply (Skinner, 1931, p. 442) and sexual cycles (Skinner, 1938, p. 416), as well as the exogenous variables we did not code (e.g., injury to the nervous system, experimentally induced brain lesions).

Mentions. Where Skinner mentioned physiology but in passing, he noted its general inaccessibility, but how this would change as biotechnology advanced (e.g., Skinner, 1973a, 1989a). In addition, he addressed the role of anatomy in behavior, for example, the interplay between a species' anatomy and its modes of behavior (e.g., key pecking by pigeons; Skinner, 1953, p. 93). More generally, though, he lamented, "The kind of science that I believe is most likely to promote an eventual integration with the physiological, biochemical, and pharmacological sciences is not very close to what is going on today in either psychology or psychiatry" (Skinner, 1959a, p. 228).

We conclude Skinner's coverage of physiology with this: "There is no doubt of the existence of sense organs, nerves, and brain, or of their participation in behavior. The organism is neither empty nor inscrutable; let the black box be opened" (Skinner, 1969b, p. 280; see Skinner, 1974, p. 233).

Evolution

As for evolution, Skinner referred to it in seven titles, discussed it substantively in eight publications, and mentioned it in 22 others. In contrast to genetics and physiology, he did not explicitly address evolution or use the term until 1950 (Skinner, 1950). Even afterward, he did not much address it until the late 1960s, after which his rate increased markedly.

Titles. Skinner's first reference to evolution in the titles of his publications was his aforementioned paper, "The Phylogeny and Ontogeny of Behavior" (Skinner, 1966a), where he notably wrote, "No reputable student of animal behavior has ever taken the position 'that the animal comes to the laboratory a virtual *tabula rasa*,' that species differences are insignificant, and that all responses are equally conditionable to all stimuli" (p. 1205; see Skinner, 1975a, 1980b, 1981, 1983a, 1984c, 1986; see also Plotkin, 1987; Richelle, 1987). He advanced this point further in discussing Breland and Breland's (1961) critique of his system: "I was only too willing to agree that the behavior of any species could not be adequately understood, predicted, or controlled without knowledge of its instinctive patterns, evolutionary history,

and ecological niche" (Skinner, 1979, p. 209).

Discussions. Although Skinner did not use the term *phylogeny* before 1966, he did discuss species status. In *Science and Human Behavior*, for example, he wrote, "Behavior is as much a part of the organism as are its anatomical features. Species status itself is a variable that needs to be taken into account in evaluating the probability of any behavior" (Skinner, 1953, p. 157). Here, Skinner also discussed how behavior and behavioral processes were selected for by their "biological advantages." For instance, in relating how a dog's leg flexed when it stepped on a sharp object, he wrote, "Such biological advantages 'explain' reflexes in an evolutionary sense; individuals who are most likely to behave in these ways are presumably most likely to survive and to pass on the adaptive characteristic to their offspring" (p. 54; see Skinner, 1983b, p. 13). In addressing the evolution of operant behavioral processes, he noted that (a) "a capacity to be reinforced by any feedback from the environment would be biologically advantageous" (p. 93), (b) the susceptibility of operant behavior to discriminative stimulus control was of "obvious biological significance" (p. 109), and (c) "the connection between reinforcement and satiation [and deprivation] must be sought in the process of evolution" (p. 83). He reiterated these points on other occasions throughout the later part of his career, writing for instance that "A means of making slight changes in behavior during the lifetime of the individual must have had survival value, and the processes of respondent and operant conditioning could evolve" (Skinner, 1983b, p. 11; see Skinner, 1969a, p. 201; 1980a).

Skinner addressed not only the evolutionary basis of the basic behavioral processes but also individual differences within them, for instance, in the "speeds with which [individuals] can be conditioned" (Skinner, 1974, p. 244) and "the rate at which changes in behavior take place" (Skinner, 1953, p. 196). On this point, Skinner (1953) commented, "the 'intelligent' individual . . . is commonly supposed to show more rapid conditioning and extinction, to form discriminations more rapidly, and so on" (p. 196) that would affect "the nature and size" of a repertoire (Skinner, 1974, p. 244).

Mentions. Among the publications in which Skinner mentioned evolution just in passing, he addressed its effects on species' repertoires (Skinner, 1972b), the occurrence of instinctive behavior (Skinner, 1973b), and the effects of instinctive behavior on operant behavior (Skinner, 1950). He also commented on the ongoing evolution of the human species (Skinner, 1971b, 1973a) and the role of biological evolution in the context of cultural evolution (Skinner, 1971c).

Overall, Skinner's view of evolution may be summarized this way: "The behavior of organisms is a single field in which both phylogeny and ontogeny must be taken into account" (Skinner, 1977, p. 1012).

Summary

The first purpose of this paper was to assess whether Skinner denied or dismissed biological participation in behavior, even to the point of avoiding organismic terms and concepts. Our findings show that he did not. Our second purpose was, in part, to assess whether he addressed biological participation only late in his career. Our findings refute this, too. Indeed, Skinner himself addressed this point on several occasions. For example, although he did not explicitly mention "evolution" until 1950, he avowed he had done so from the start. For example, about genetics, he wrote, "Several commentators refer to my 'recent' interest in the genetics of behavior, but my interest is actually longstanding" (Skinner, 1984a, p. 701). About ethology, he commented, "Ethologists often assert that their work is neglected by behaviorists, but Watson's first experiments were ethological, and so were mine"

TABLE 1

Chi-squares across the expected and actual number of times Skinner addressed genetics, physiology, and evolution by content domain and extent (Part A) and across the expected and actual number of times he addressed them, controlling for his changes in base rate (Part B)

Part			Quarter 1	2	3	4	χ^2
A	Total	Expected	72	72	72	72	
		Actual	51	53	86	92	19.4**
	Biology	Expected	24	24	24	24	
		Actual	14	19	28	33	9.2*
	Genetics	Expected	15	15	15	15	
		Actual	8	9	24	18	11.5**
	Physiology	Expected	9	9	9	9	
		Actual	6	13	6	10	3.9
	Evolution	Expected	9	9	9	9	
		Actual	0	4	11	21	28.2**
	Titles	Expected	4	4	4	4	
		Actual	7	1	3	6	5.4
	Discussions	Expected	6	6	6	6	
		Actual	1	3	4	14	18.4**
	Mentions	Expected	24	24	24	24	
		Actual	6	22	34	29	19.1**
B	Actual publications		51	53	86	92	
	Proportion of actual		18%	19%	31%	33%	
	Biology	Expected	18	18	30	32	
		Actual	14	19	28	33	0.9
	Genetics	Expected	11	11	19	20	
		Actual	8	9	24	18	3.1
	Physiology	Expected	7	7	11	12	
		Actual	6	13	6	10	8.3*
	Evolution	Expected	7	7	11	12	
		Actual	0	4	11	21	14.9*
	Titles	Expected	3	3	5	6	
		Actual	7	1	3	6	7.5
	Discussions	Expected	4	4	7	7	
		Actual	1	3	4	14	10.1*
	Mentions	Expected	17	18	29	31	
		Actual	6	22	34	29	9.3*

* $p = .05$, $df = 3, 7.82$.
** $p = .01$, $df = 3, 11.34$.

(Skinner, 1980a, p. 199). And, about evolution, he wrote, "As I have pointed out elsewhere, my 'late' interest in evolution began with the first five pieces of research I ever undertook" (Skinner, 1984a, p. 703). Among these publications are ones we have already cited—Barnes and Skinner (1930) and Skinner (1930a, 1930b, 1932a, 1932b) (see also Heron & Skinner, 1937, 1940; Lambert et al., 1933). Skinner's avowals, however, do not refute the claims that he addressed biological participation more often later in his career than earlier. We address this point next.

WHEN SKINNER PUBLISHED

Table 1 numerically summarizes the cumulative curves presented in Panel A of Figure 1 with numbers and percentages across roughly four quarters of Skinner's career—1930 through 1944, 1945 through 1959, 1961 through 1975, and 1976 through 1990.[1]

[1] To facilitate the analyses across the four

In Part A, we see that Skinner addressed biological participation significantly more often later in his career than earlier—genetics and evolution in particular, but not physiology. This was also true of his discussions and mentions, but not his titles. Although these data support the claim that Skinner addressed biological participation more often later than earlier, they do not take into account his base rate of publication: 63% (not 50%) of his works appeared in the second half of his career, 33% (not 25%) in the last quarter. Part B adjusts for this by showing Skinner's increasing base rate of publications in frequencies and percentages, and then the expected number of publications, given these percentages, compared to the actual number. The statistical comparisons here show that Skinner did not, in general, address biological participation more often later than earlier. Moreover, although evolution increased significantly, genetics did not, and physiology was statistically significant in the opposite direction.

WHY SKINNER PUBLISHED

When Skinner addressed biological participation more often later in his career than earlier, his critics argue that this was due to the overwhelming evidence for it or to the mounting criticism that he had overlooked it. Although none of our findings support these arguments, they do not count against them either. What would count against them is evidence that other factors affected Skinner's increasing attention to biological participation. For this, we searched the literature for both general conditions and specific events in his career, psychology, and biology that would account for this change.

General Conditions

In perusing Skinner's autobiographical writings (e.g., Skinner, 1967, 1979, 1983b), we found that, as his career progressed, he focused less on his science of behavior and more on his system of psychology. This is supported by Coleman's (1982) analysis of Skinner's publications between 1928 and 1978. In the first two of these decades (1928 to 1948), Skinner published 72% of his scientific works but only 7% of his systematic works, whereas in the last two of these decades (1958 to 1978), the figures were 15% and 43%, respectively. In addition, Skinner became a public intellectual as his career progressed (see Posner, 2001) which, along with his becoming a systematist, might have led him to emphasize the generalities of his system over the particulars of his science, among them, more inclusive accounts of the variables of which behavior was a function, biological participation included.

Psychology and science changed, too. For instance, over the second half of Skinner's career, psychology became more explicitly cognitive (Baars, 1986; Lachman, Lachman, & Butterfield, 1979). However, in staving off criticisms that cognitive psychology was literally dualistic, most cognitive psychologists turned to neuroscience as the basis of mind (e.g., Churchland, 1986). This did not go unnoticed by Skinner (1983b, p. 367), in which context he increasingly compared and contrasted how he and his cognitive colleagues addressed biological participation (see Skinner, 1985, 1990a). Finally, in science more generally, the field of biology burgeoned in the last half of Skinner's career (Cohen, 1985, pp. 384–385; Parkins, 1966). As a result, he was more likely to discuss it later than earlier in his career, just as were his critics.

Specific Events

Turning to events specific to Skinner's career, and in psychology and science, we find still further explanations

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quarters of Skinner's career, we put Skinner's 1960 publications aside for statistical purposes, leaving the 60 years of his career divided into four 15-year periods. Among Skinner's seven publications in 1960, which were included in Figure 1 and all other data summaries, three mentioned biological participation in the context of genetics and physiology.

for his increasing attention to biological participation. They began at about the midddle of the 20th century.

About midcentury. As can be seen in Panel C of Figure 1, Skinner's rate of addressing genetics and physiology increased around midcentury. Several events may have precipitated this. First, before 1950, ethology was largely a European science, but by midcentury it was introduced to North American psychologists through such books as the English edition of Tinbergen's (1951) *The Study of Instinct* and Lorenz's (1952) *King Solomon's Ring* (see also Dewsbury, 1984, pp. 131–168; Lorenz, 1950, 1961, 1965). These engaging texts addressed species-specific patterns of adaptive behavior—for example, nesting, imprinting, and aggression—that would have to be addressed by American psychologists interested in animal behavior, Skinner included. In fact, by the late 1950s, he himself was conducting research on instinctive aggression in pigeons (Skinner, 1983b, pp. 157–158; see Reynolds, Catania, & Skinner, 1963).

Second, the Hungarian ethologist Schiller (1952, 1957; see Dewsbury, 1996) wrote Skinner in 1949 from the Yerkes Laboratory of Primate Behavior, asking if he could work in his laboratory to familiarize himself with operant techniques. Skinner (1983b, p. 14) noted that Schiller had already provided phylogenetic and ontogenetic accounts of "insight" in apes (as opposed to Gestalt accounts) and welcomed him to Harvard that fall. Schiller immediately began a program of research on unconditioned and conditioned "attack" in Siamese fighting fish, but unfortunately never completed it; he died in a skiing accident that spring. Nonetheless, his correspondence with Skinner, and his presence in Skinner's laboratory, might reasonably have led Skinner to address ethological considerations more often then than before.

Third, Verplanck, Skinner's colleague at Indiana and Harvard, spent the spring of 1953 at Oxford University with Tinbergen and met Lorenz. He returned to Harvard, Skinner (1983b) wrote, "full of the new discipline" (p. 101; see Verplanck, 1955). When Skinner (1983b) was asked in 1953 whether Lorenz should be invited to lecture at the Harvard Medical School, he replied that Lorenz would be "immensely stimulating" (p. 101). While at Harvard, Lorenz visited Skinner's laboratory, as did the comparative psychologist Lehrman (1953; see Skinner, 1983b, p. 207).

A fourth precipitating event may have been J. D. Watson and Crick's (1953) discovery of the structure of DNA—the double helix (see J. D. Watson, 1968). In the 1930s and early 1940s, the "modern synthesis" had integrated Darwin's (1859, 1871) account of natural selection with the principles of modern genetics, but the significant works of that time—Dobzhansky's (1937) *Genetics and the Origin of Species*, Huxley's (1942) *Evolution, the Modern Synthesis*, and Mayr's (1942) *Systematics and the Origin of Species from the Viewpoint of a Zoologist*—seemed not to have influenced Skinner, perhaps because he was then concentrating on his own science. Watson and Crick's discovery, though, was a singular event in the history of science and much discussed in the behavioral sciences and popular press (Cohen, 1985, pp. 384–385; Medawar, 1977). It, too, might have increased Skinner's likelihood of addressing biological participation. This was also about the time Skinner was finishing *Science and Human Behavior* (1953), his first general extension of his science into a psychological system. This would require that he explicitly address biology even further, which he did, as illustrated in the many quotations we drew from his book.

After midcentury. In the late 1960s and early 1970s, Skinner again increased his rate of addressing biological participation—evolution, in particular. One or a combination of factors might explain this. First, as previously noted, Skinner was not only becoming

a systematist and a public intellectual, but he was also extending his work into human affairs and values in such books as *Beyond Freedom and Dignity* (Skinner, 1971c) and *About Behaviorism* (Skinner, 1974). In describing the individual, social, and cultural implications of his science and system—and urging deep changes in the behavioral, social, and cognitive sciences—he was presumably led to address biological participation more then than before.

Second, the "constraints on learning" literature also emerged at this time (see Hinde & Stevenson-Hinde, 1973; Seligman & Hagar, 1972; Shettleworth, 1972). In contrast to the received view, which purportedly held to the continuity, equipotentiality, and contiguity assumptions, this literature demonstrated, respectively, that (a) the processes or parameters of learning varied across species, (b) not all stimuli and responses were equally conditionable, and (c) close temporal contiguity was not necessary for conditioning. This literature was much discussed in psychology, making Skinner likely to address it (e.g., Skinner, 1977). When the ethologists Tinbergen, Lorenz, and von Frisch won the Nobel Prize in Physiology or Medicine in 1973, he might have been even further spurred to relate his work to ethological considerations (e.g., Skinner, 1975a).

Third, during this period, Skinner was increasingly criticized for denying or dismissing biological participation in behavior and for holding the received view on continuity, equipotentiality, and contiguity. Although he usually did not respond to his critics, he did on these topics, in particular, on what he saw as an "ethological attack" on his system (Skinner, 1983b, pp. 208, 230) and misunderstandings of his views (see, e.g., Garcia, 1981; Garcia & Garcia y Robertson, 1985; Gould, 1982; Gould & Marler, 1987). For instance, "The Phylogeny and Ontogeny of Behavior" (1966a) was partly a response to critics who, he wrote later, "had pounced upon the Breland paper with delight" (Skinner, 1983b, p. 285). "Herrnstein and the Evolution of Behaviorism" (1977) was devoted, in part, to distancing his system from the received view on biological constraints (Skinner, 1983b, pp. 374–375). "The Species-Specific Behavior of Ethologists" (Skinner, 1980b) was a response to B. R. Moore and Stuttard's (1979) claim that "most research in instrumental (operant) conditioning has been conducted without regard for the natural behavior of the animals used as subjects" (p. 1031). And, in "Selection by Consequences," Skinner (1981) addressed criticisms by the ethologists Tiger and Fox (1971) that he had neglected genetic endowment (Skinner, 1983b, p. 384; see also Todd, 1987a, 1987b). Given that these publications were responses to his critics, and that three of them were published during the last quarter of his career, we may indeed conclude that Skinner did increasingly address biological participation because of his critics, but not for the reasons they proffered.

DISCUSSION

Having now brought some data to bear on whether, when, and why Skinner addressed biological participation in behavior, we relate them to the issues we raised in our introduction and to our methodology.

First, the data do not support the criticisms that Skinner denied or dismissed biological participation or avoided organismic terms and concepts. In fact, the data underestimate the number of times and extent to which Skinner addressed them, due to the nature of our literature search, coding conventions, and the material we coded. For example, (a) although we looked through Skinner's every primary-source publication, we scanned them more than we read them word for word. Thus, we likely missed some discussions and many mentions of biological participation. (b) We did not include multiple discussions or mentions within the same content category,

or any mentions if the same category was discussed. Thus, we did not code all the occasions on which Skinner addressed biological participation. (c) We did not code Skinner's numerous reprintings of his publications (i.e., Skinner, 1959b, 1961a, 1968, 1969a, 1972a, 1978, 1982, 1984b, 1987, 1989b). For example, of the 97 we coded, Skinner reprinted six of those that had relevant titles 11 times and the other publications 99 times. (d) We did not search or code Skinner's duplicate publications (e.g., Skinner, 1975b), their reprintings by others (e.g., Skinner, 1973c), or published excerpts from his books (e.g., Skinner, 1971d). (e) By restricting our search to Skinner's primary-source publications during his career, we excluded two posthumous publications (Skinner, 1993, 2004) and the last edition of *Cumulative Record* (Skinner, 1999), all of which addressed biological participation. (f) As we mentioned earlier, we did not code for exogenous biological variables (see, e.g., Heron & Skinner, 1937; Skinner, 1931, 1938, 1959a; Skinner & Heron, 1937).

The second issue we raised in the introduction was the claim that Skinner addressed biological participation later in his career, not earlier. Our data refute this, too. Relevant titles and substantive discussions are found in his first and last publications and in 47 years of his 61-year career. However, the claim that he addressed it more often later than earlier is true. Except for evolution, though, this is accounted for by the overall increase in his base rate of publication. Indeed, in some areas, he published less later in his career than earlier.

Third, as for the argument that Skinner addressed biological participation only because of the overwhelming evidence for it or the mounting criticism that he had overlooked it, we found no support. What we found, instead, was evidence that other factors accounted for his increase. These included general conditions and specific events in Skinner's career, psychology, and science, for instance, his turning from his science to his system, his interactions with ethologists, the constraints-on-learning literature, the burgeoning field of biology, and the growing number of misunderstandings about his views.

Although our data refute the foregoing criticisms, claims, and arguments, we have not addressed the fact of their very existence. This, however, would take us beyond our purview; it deserves its own independent treatment. Nonetheless, in reading Skinner and his critics, we discerned several reasons for the misunderstandings, which we offer as a basis for future research. Among the reasons are (a) purely intellectual disagreements between Skinner and his critics, not on the facts of biological participation, but on physiological reductionism or eliminative materialism (e.g., Churchland, 1986; see J. Moore, 2002); (b) disciplinary isolation, in particular, between operant and nonoperant psychology (Coleman & Mehlman, 1992; Krantz, 1972; see Lee, 1989); (c) misreadings in the history of psychology, for instance, the mistaken historical continuity between J. B. Watson's (1924) purported environmentalism and Skinner's system of psychology (e.g., Pinker, 2002, p. 20; see Todd & Morris, 1992); (d) scholarship based on tertiary sources and academic folklore (e.g., Gould & Marler, 1987; Mahoney, 1989; see Catania, 1991); (e) general professional and disciplinary enmity toward behaviorism, Skinner included (e.g., Garcia, 1981; Garcia & Garcia y Robertson, 1985; see Lubek & Apfelbaum, 1987); (f) the vicissitudes of personal and social motivations in science (see Mahoney, 1989; Proctor & Weeks, 1990a, 1990b; cf. Catania, 1991; Wolf, 1991); and (g) the role of rhetoric—the Skinner controversies are fueled by more than positivist facts and logic alone (e.g., Black, 1973; Chomsky, 1959; see Andresen, 1990; Czubaroff, 1988; Sherrard, 1988).

Additional reasons may lie in Skinner himself, however. First, although he addressed biological participation

throughout his career, he mentioned it much more often in passing than in substantive discussions. He may simply have not addressed it deeply, often, or clearly enough to offset the reasons mentioned above. Second, he sometimes wrote in ways that invited misunderstanding. For instance, in a discussion of shaping, he noted, "Operant conditioning shapes behavior as a sculptor shapes a lump of clay" (Skinner, 1953, p. 91). In presenting nearly identical cumulative records from three species, he asked, "Pigeon, rat, monkey, which is which?" His answer: "It doesn't matter . . . their behavior shows astonishingly similar properties" (Skinner, 1956a, pp. 230–231). And, in addressing the variables that were immediately available for the analysis of behavior, he said they "lie outside the organism, in its immediate environment and in its environmental history" (1953, p. 31).

These statements suggest that Skinner held to the continuity and equipotentiality assumptions, and to a radical environmentalism, even though these were not his views, as seen in the broader corpus of his work. Being an expert in all of Skinner's writings, however, should not be a prerequisite for correctly interpreting selected aspects of his system. In any event, as to the foregoing quotations: (a) The shaping–sculpting metaphor concerned the "continuity of behavior" (Skinner, 1953, pp. 91–98), not nature and nurture (Skinner, 1983b, p. 103); (b) the similarity of schedule effects concerned "performances, not entire repertoires" (Skinner, 1984b, p. 508); and (c) the variables of which behavior is a function lie outside behavior, but not outside the organism—"part of the universe is enclosed within the organism's own skin" (Skinner, 1953, p. 257; 1973a, p. 260).

These reasons for the criticisms, claims, and arguments that Skinner denied or dismissed biological participation do not, of course, exhaust the possibilities. The very certainty of Skinner's critics itself invites deeper and subtler analysis. Where these misunderstandings are merely symptomatic of less than impeccable scholarship (e.g., his critics not knowing what they do not know), then our bringing some data to bear on them may improve the conditions for future work. More important consequences are at stake, however. An accurate appraisal and appreciation of Skinner's stance on biological participation may (a) improve the professional relations among behavior analysts and their colleagues in the behavioral, social, and cognitive sciences, especially in behavioral neuroscience and ethology and (b) facilitate the interdisciplinary and transdisciplinary research necessary for understanding behavior. As Skinner noted: "Human behavior will eventually be explained (as it can only be explained) by the cooperative action of ethology, brain science, and behavior analysis" (Skinner, 1989a, p. 18).[2]

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[2] Although our findings reveal that Skinner addressed biological participation on many occasions, we are not suggesting that his views were other than conventional, for instance, his traditional parsing of nature and nurture (e.g., Skinner, 1966a). This dichotomy remains standard in the behavioral, social, and cognitive sciences, as it is in behavior analysis (cf. Reese, 1995) and in Lorenzian versions of ethology (see Johnston, 1982). As for ourselves, we are drawn to nonfacile forms of developmental systems theory in understanding how biology participates in behavior (e.g., Gottlieb, 1997, 1999; Oyama, 1985; Oyama, Griffiths, & Gray, 2001). We do not, however, unduly criticize Skinner in these matters: Developmental systems theory was not broadly advanced until late in his career. Even now, it is not well understood, and has only recently been addressed in the behavior-analytic literature (Midgley & Morris, 1992; Schnieder, 2003).

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